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ORIGINAL ARTICLES.

SOME PECULIARITIES IN THE REFRACTION BY TILTED LENSES, GRAPHICALLY REPRESENTED.

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The fact that an astigmatic refraction followed from a position of a spherical lens oblique to the incident pencil has been known for a long time, the instance most familiar to ophthalmologists being the well-known one of Dr. Thos. Young, who demonstrated his ocular astigmatism as due to an oblique position of the crystalline lens, relative to the optical axis of the eye. The subject was first treated of, experimentally and mathematically, in this country, by Pickering and Williams[1], in 1875. Of late, attention has again been called to it from a mathematical standpoint by Holden[2] and by Green.[3]

Neither of these two latter contributions, while in the main accurate and carefully worked out, contains anything essen-

[1]Proceedings Amer. Association of Arts and Sci., 1874-75.

[2]Knapp's Archives, 1891.

[3]Amer. Oph. Soc., 1890.

tially new, or what cannot be found in the works of Heath[4] or Aldis[5], where, indeed, the matter is handled in a most thorough manner, mathematically. Holden, in his history of the literature of the subject, makes no mention of these works; an omission we make good here, not only on account of this special instance, but also because the student of optics will find in them, probably, the best guides to be had in the English language in working out many of the problems with which he has to deal.

While preparing my "Treatise on Astigmatism" (1887), I made a number of experiments with the phakometer of Snellen to verify the findings of Pickering and Williams, and in the course of these investigations discovered some phenomena to which I had nowhere found an allusion. My discovery was[6] that while in a tilted spherical lens of our ordinary trial cases, there was an astigmatic action with two focal areas, and what corresponded to a focal interval of Sturm, these focal areas were not, or at least the posterior was not, perpendicular to the optical axis, or the incident ray. The posterior focal area was, on the contrary, oblique to the line of direction of the incident ray and in a direction opposite to the obliquity of the lens; that is, if the lens were inclined 30° in one direction, the screen on which the image falls must, in order that the image approach most nearly to a line, be rotated, not so as to be parallel to the lens, but 30° in the opposite direction, so that planes passing through each would meet, if prolonged. So far as I am aware, this special phenomenon has not been treated of mathematically, nor is it even referred to by Pickering and Williams, or Holden or Green, and only in a very general manner by Heath and Aldis. It is readily understood that this would be a difficult if not an impossible problem to deal with by the ordinary formulæ, because these, as laid out by Gauss and his followers, deal only with lenses of limited am-

[4]Geometrical Optics, Cambridge, 1889.

[5]Geometrical Optics, Cambridge, 3rd edition, 1888.

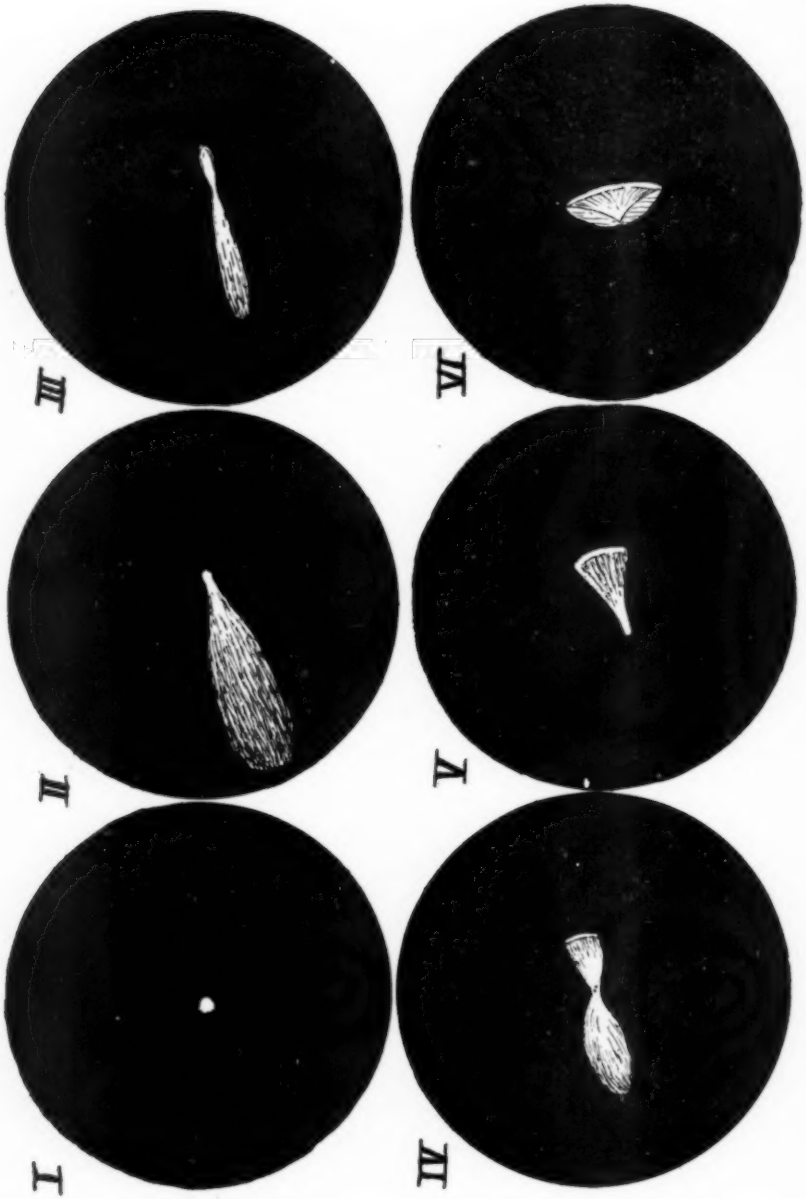
[6]Published at the time in the Amer. Jour. of Ophthalmology for January, 1887.

plitude, in which the arc, sines and tangent are considered as the same. It is evident that in tilted lenses these simple formulæ no longer apply, and for their mathematical elucidation much more elaborate equations will be required. It is also true, I think, that the phenomena are due largely to the causes which produce ordinary spherical aberration, though the use of diaphragms, unless very small, does not do away with the appearances entirely.

Since this paper has been put in type, Mr. C. F. Prentice has repeated the experiments and confirmed the phenomena; and shown, by construction, that they are due, as above indicated, to the laws of spherical refraction operating on a surface of large amplitude. It is hoped that the diagrams illustrative and explanatory of this may be published soon.

Pending, however, a mathematical solution, I have thought it would not be without interest to give a representation of the forms of the focal area of a point of light, the rays from which are refracted by a spherical lens in one special tilted position. The lens chosen is 10 D., from the test-case, and in Fig. I. we have the focus at 10 cm. of a gas jet, 20 feet away. Fig. II. is the focal area when the lens is rotated 35° on its vertical axis, the screen remaining at right angles to the line of direction of the incident rays. Fig. III. shows this area, when the screen is placed at an obliquity of 35° in a direction opposite to the inclination of the lens. Fig. IV. is the focal area on the screen when it is at right angles to the direction of the incident pencil, but at 9 cm. from the center of the lens. Fig. V. is the form it has at 8 cm., and Fig. VI. gives its form at 7 cm.

In studying the changes in the form of the focal area it is to be noted that instead of being horizontal it has a direction downward and to the left (the left edge of the lens being nearest the screen). As the screen approaches the lens it assumes a perfect screw surface in Fig. IV., the portions of greatest brightness being near the center, and a bright line bounding the right extremity. This bright line at the right end remains in all the figures, but the other one, as is seen, shifts to the


III
VI
II
V
I
IV

left extremity in Fig. V., and forms the apex of the brighter cone inside of the oval area, in Fig. VI.

As the screen is brought nearer than 7 cm. the oval of Fig. VI. broadens out into a large area of diffusion, in which the details are no longer to be made out. Fig. VI. represents what would be the anterior focal line in Sturm's interval, but it is a question as to whether one shall look upon II. or III. as the posterior boundary of the interval.

We could have used a more complicated and delicate apparatus and have secured, in that way, more and finer details, but what we wished to give was the refractive images in the tilting of an ordinary lens, as it would be used in the form of spectacles. The experiment is extremely simple and can be repeated by any one, with only a little care and trouble.

SOCIETY PROCEEDINGS.

REPORT OF THE DISCUSSIONS OF THE 24TH ANNUAL MEETING OF THE AMERICAN OPHTHALMOLOGICAL SOCIETY, HELD AT WASHINGTON, D. C., SEPT. 23 AND 24, 1891.

Paper by Dr. D. Webster: "Report of Cases of Cataract Extraction." Discussion postponed until all papers on the same subject had been submitted.

Paper by Dr. H. Knapp: "The Occurrence, Prevention and Management of Prolapse of the Iris in Extraction of Cataract.'

Paper by Dr. J. A. Lippincott: "Routine Syringing-out of Cortical Matter in Cataract Extraction," as illustrated by one hundred cases.

Discussion.

Dr. Emil Gruening, of New York.—Dr. Knapp said in his paper that the old operators cut the Gordian knot. They had no cocaine in those days. Those who operated in the days when cocaine was not used will remember that the iris always prolapsed when they opened the anterior chamber, and this was a welcome sight, because they wished it to prolapse, and then cut it off. Now it does not come out but remains in the anterior chamber, and we want it; therefore I cannot see that we are in the position of the old operators. We are not obliged to cut it. The iris remains in the eye because there is hypotony of the eye induced by cocaine. The gentleman who read the first paper on cataract says that the cocaine

should be instilled until the dilatation of the pupil comes on. Now it is known that if we use a 4% solution of cocaine in order to produce dilatation, we shall have to wait at times fifteen minutes, at times even longer. If we instill a 4% solution of cocaine every fifteen minutes we shall often have such hypotony that the lens may not come out in spite of well-directed pressure upon the eye. Those who have had this experience will know how important it is to understand thoroughly how to cocainize. Dr. Knapp says, "cocainize thoroughly." That is not sufficient. How thoroughly? What is a thorough cocainization when we wish to operate. We wish to produce sufficient hypotony to act upon the iris in the eye and still not cause a hindrance to the exit of the lens. I have found that a 4% solution instilled fifteen minutes before the operation caused excessive hypotony in most cases; that instillation even for ten minutes before the operation causes too much hypotony, and I have reduced it to six minutes, and during these six minutes cocaine is instilled three times. Then I get exactly what I want. Should eserine be instilled after the operation? I think eserine should be instilled after every simple operation for cataract. One per cent produces iritis in every case. A one-fourth per cent solution answered best in my cases. Even then a slight iritis could be observed, but a few slender adhesions are not only harmless, but really useful in retaining the iris in its position until the corneal wound is sufficiently firmly closed.

DR. C. S. BULL, of New York.—I think, and all who have done this operation will, I believe, agree with me, that prolapsus of the iris is the one vicious factor in simple extraction, and that our labors in the future should lessen the frequency of its occurrence. I agree with Dr. Knapp absolutely as to the necessity of locating the corneal incision entirely within clear cornea just touching the limbus, the base line of the flap being just above or just below the horizontal meridian. I do not agree with him in the turning of the knife backward in order to form a small conjunctival flap because I believe a conjunctival flap is a vicious thing to make. I do not agree

with Dr. Knapp in his belief as to the effects of the eserine, nor am I as positive as Dr. Gruening is in regard to the danger of a one per cent solution of eserine always causing iritis. I have known very severe iritis in very many cases to be induced by a one-half grain solution. I never use a stronger solution than a one-half grain and usually employ a solution of one-tenth of a grain. This does not produce iritis and it does produce what we want. I use eserine in every case after the operation.

As regards the dressing of the eye, I agree with the reader of the paper that the plaster should be avoided. In almost all cases in my practice—it is a routine thing, I may say—I use a double-roller bandage in all operations for extraction of cataract, making the bandage of very thin material, particularly in very warm weather, and leaving it on one or two days. I still think that one of the strongest means we have for avoiding the occurrence of prolapse is to do the operation while the patient is in the bed in which he is to remain during his convalescence. That is the strongest means that I know of to avoid the occurrence of a prolapse. And one point in addition: When the prolapse happens, I believe in as early and as complete an excision of that prolapse of the iris after your operation as possible.

DR. HENRY D. NOYES, of New York.—I have studied very carefully this subject of prolapse of the iris, and I think that one point deserves more explicit mention than has been given to it in our discussion; in fact, it has been entirely omitted. Prolapsus of the iris is not an accident exclusively belonging to the simple operation. Cases of operation with iridectomy are accompanied by the same accident, only the prolapsus cannot affect the iris in the middle of the wound, but it presents itself at its angles, and everybody who has been accustomed to von Graefe's operation knows that incarceration of the iris is one of the ominous and serious occurrences belonging to that method of procedure, as it gives rise to cystoid cicatrices and to other complications. A recent writer in the last number of *Knapp's Archives of Ophthalmology*, reported

two hundred cases and puts the ratio at four or five per cent of the number as those in which it was found. I think if we should go over our old cases we should find that at least as much as five per cent of them were accompanied by incarceration ofthe iris at the angle of the wound.

But to come to the matter in simple extraction, I have summed up a number of cases as they have occurred to me, of this accident, from which I make the following statement: That a condition of plus tension at the time of operation is the first and most serious consideration, and that this plus tension is something as like a tendency to simple glaucoma inevitably invites prolapsus. It may or may not be overcome by cocaine, but if at the time of the operation there be plus tension you will get prolapse. The second condition which gives rise to it is the uncontrollable squeezing of the patient. A recent experience of that kind came to me in two negroes who came to the New York Eye and Ear Infirmary, and who made very powerful compression of the eyes by the eyelids. I operated on one eye at a time in each and both had a very large and copious prolapse of the iris. The operations were normal, but the squeezing was persistent and uncontrollable.

The third thing which seems to me to render this prolapse of the iris extremely liable is a rude operation; that is an operation accompanied by a certain amount of violence, thereby producing more or less reaction in the healing process, and I am sure that this favors the unhappy occurrence of the prolapsus.

The last is the traumatism. I am certain, as Dr. Knapp has said, that traumatisms to which we pay little attention are responsible for a great many of these occurrences, and not only within the first two days does it make its appearance, but at the expiration of six or nine days may it appear by reason of the secondary opening of the wound. I have seen that take place several times.

DR. SAMUEL THEOBALD, of Baltimore.—I wish to say just one word with regard to the production of iritis by the use of eserine. I am very glad to hear that Dr. Knapp, from his lat-

ter experience, believes that iritis is not apt to be produced by even the one per cent solution of eserine. If I remember correctly, both Dr. Knapp and Dr. Bull agreed at the last meeting of the Ophthalmological Society, that from their experience up to that time iritis was frequently produced by eserine, and that very weak solutions of eserine must be used if we wished to avoid iritis. At that time I made the remark that my experience had been quite different; that I had usually employed a solution of the strength of four grains to the ounce, and that iritis had been an extremely rare occurrence.

If I am right in my recollection of what Dr. Knapp's earlier experience was, I congratulate myself that this later experience of his so closely coincides with my own.

DR. REEVE, of Toronto.—At the discussion before the Society last year, I expressed the opinion that iritis was frequently produced by a bruising of the iris during exploration and not by eserine.

DR. W. V. MARMION, of Washington, D. C.—In the first year of my professional career, I practiced the flap extraction without iridectomy, using the Beer knife to make the section. The various accidents attendant upon this form of operation, particularly suppuration of the cornea, led me, as it did, I may say, every one, to adopt Graefe's modified linear procedure, and it is this method that I follow to-day, with one modification. I always make a small upward iridectomy from three to six weeks in advance of the extraction of the lens as originally suggested I think by Mooren of Duesseldorf, taking care that the iris is not caught, to the slightest extent, in the angles of the wound; and my results are so very satisfactory that I can see no reason for changing to the method under discussion, which is simply a revival of the flap operation, the narrow Graefe knife being used instead of the Beer. I practiced this method five times two years ago, and had prolapse of the iris in two of the cases on the third day. Abscision was done in one case; and was followed by intense reaction and impairment of vision, the prolapse re-forming after a few days. I enlarged the coloboma and the hernia being reduced,

I passed a flat needle into the fistulous opening and gently scraped the edges, securing complete union in a few days. It seems to me that one cause of prolapse of the iris is the concaving of the corneal lip of the flap when making the section, in part at least, down to the membrane of Descemet, thus preventing perfect coaptation of the edges of the wound. Cocaine in a four per cent solution frequently toughens the cornea to such an extent that it is almost impossible to puncture it. I find a two per cent solution answers much better.

Of eserine in the treatment of cataract, I have had no experience, but I have seen destructive iritis follow its use in the treatment of glaucoma, the solution being one grain to the ounce.

DR. H. KNAPP, of New York.—With regard to eserine, I believe with Dr. Reeve that iritis is frequently due to the bruising of the iris during the operation. I believe with Dr. Bull and Dr. Gruening that eserine favors the occurrence of iritis, but when there are no anatomical conditions to cause iritis, a 1% solution of eserine may be instilled without fear. The mode of preventing iritis is to avoid bringing two raw surfaces together, the lacerated capsule and the lacerated pupillary edge of the iris. Those who make peripheric capsulotomy will notice synechiæ only at the place where the capsule is opened. During the passage of the lens through the pupil the free edge of the iris is stretched and lacerated in several places which can easily be seen. These lacerations become invisible when the iris is reduced and gives rise to posterior synechiæ only when the lens capsule is opened in the center. When the lens capsule is incised at the periphery only, the instillation of 1% solution of eserine produces no iritis, a fact which I can demonstrate any day at the hospital.

Paper by Dr. G. R. de Schweinitz, of Philadelphia: "A Case of Acute Glaucoma Occurring in the Eye with Coloboma of the Iris and Supernumary Pupils," Illustrated.

Paper by Dr. B. L. Milliken, of Cleveland, O.: "A Partially Bony Growth of Orbit—Removal—Recovery."

Paper by Dr. C. S. Bull, of New York: I. "Gout Retinitis." 2. "Neuro-Retinitis and Chorio-Retinitis."

DR. HEYL, of Philadelphia.—With reference to this Schoeler operation, of course it is a matter which interests us very much because it deals with a very dreadful disease. I have gone over Schoeler's monograph on the subject, and have come to the conclusion that the remedy is about as dreadful as the disease, and the experience of Dr. Bull will go far to confirm me in this conclusion. So that for myself I am utterly disinclined to attempt it at all. I am not sure that the pathological principle upon which the operation is based is correct. I do not deny the existence in retinal separation of the tear through the retina or of the vitreous bands, but I am rather inclined to think that that is not the whole of the process, and I believe that we shall never get at the true rationale until we understand the principles underlying the tension of the ball. Meanwhile I believe that the procedure had better be done away with.

DR. T. R. SUTPHEN, of Newark, N. J.—May I ask Dr. Bull if there was a suppression of the intracranial fluid?

DR. BULL.—No sir.

DR. T. Y. SUTPHEN.—I recall three operations in cases of this kind which I reported to this Society in 1888; one of which was eminently successfnlly, the operation being a simple puncture. That case has good vision to this day. He is slightly myopic with vision $^{20}/_{XXX}$.

DR. DAVID WEBSTER of New York.—I want to thank Dr. Bull for making these experiments for us. One thing I would like to an inquire about. He reports that in two cases panophthalmitis occurred, and yet the resulting vision was very considerable. Now he must put a different meaning to the term panophthalmitis than that which I have learned. I have always thought that a case of panophthalmltis left nothing beyond a mere perception of light.

DR. BULL.—These were cases of intra-ocular inflammation starting in the vitreous or choroid, and so far as it was possible to judge from external manifestations involving all the principle tissues of the eyeball. There was enormous chemo-

sis, enormous swelling of the eyelids protrusion of the eyeball, excessive pain and increases of temperature.

DR. SAMUEL THEOBALD.—I think that all of us have seen cases of at least traumatic panophthalmitis where with the patient seen early enough by rapid and persistent efforts we have reduced the inflammation so that the trouble lasted only a few days, and left a certain amount of vision.

DR. C. S. BULL.—Dr. Webster is mistaken in stating that I reported that the vision was good in these cases. It was bad, very much worse than it had been before the operation was attempted.

DR. SAMUEL J. JONES, of Chicago.—May there not be a factor of disturbance in the use of the iodine?

DR. C. S. BULL.—The iodine being used as an antiseptic and preventing the process which is generally the result of panophthalmitis, that may explain the difference between the destruction of the tissues which we have seen and the report of the cases as given.

Paper by Dr. Hasket Derby, of Boston: "The Significance of Macular Changes in Advancing Myopia."

DR. HENRY D. NOYES.—I would like to say one word upon this subject: We see this condition often in eyes that are not myopic. It is by no means an uncommon experience with me to have persons of advancing age, as they approach sixty for example, to exhibit just such changes at the macula that are so commonly seen in cases of myopia; and this in most instances appears to depend upon general failure in nutritive power; it occurs most frequently in emaciated individuals, though I have seen it occur in persons who were quite corpulent. These cases seem like simple erosion, and absorption of the tissues at the center of the eye and they have in my observation been just as hopeless as are the cases which occur in myopia. The prognosis is usually that they go on to a gradual deterioration of sight but I have never seen them lose all sight. The scotoma becomes more and more marked and I have rarely seen an arrest of the progress of the disease take place. The usual thing is that they go on to a gradual deter-

ioration, without absolute loss of vision at this part. But the prognosis is bad, and the treatment is of no avail.

DR. ALBERT G. HEYL, of Philadelphia.—I would like to say a few words with reference to the genesis of appearances of the kind described. They are usually ascribed to a form of choroiditis, but I wish to call attention of members to the effect of simple pressure as a cause. We all know there must be a certain amount of vitreous pressure in every eye, and the appearances from the effect of blows upon the eye (rupture of the choroid) show that when the force bears upon the *pigmentary* layer of the retina, then we draw the conclusion that the vitreous pressure bears upon the pigmentary layer. (? Editor).

For a fuller discussion, see The Visual Axis, by Albert G. Heyl, M.D., Transactions of the 9th International Medical Congress, Vol. III, p. 740.

Now it is impossible in three minutes to fully develop this, but in intrauterine life I believe that vitreous pressure bears upon the primordial retina and as a result the macula is formed by the splitting of the retina; but in extra-uterine life it bears especially upon the pigmentary layer of the retina, and if the vitreous tension be great, you may have a local injury or you may have a distributed injury disintegrating the pigment to a large degree. I believe that; in myopia you may see the same sort of pigmentary changes, and instead of being a true inflammatory condition, I believe that it is one of pressure. And you may have one of two forms; either localized, affecting the macula itself, or you may have the appearances such as have been described or distributed over a large section of the pigmentary area with a large destruction of pigment.

DR. SAMUEL D. RISLEY, of Philadelphia.—My observation of these macular changes in eyes with defects of refraction, particularly astigmatic eyes, has led me to regard them as but a part of the pathological history of the distending or stretching eye, and may be present before the eye has reached the stage of myopia. My experience in their treatment, however, does not fully accord with the very grave opinion expressed by Dr.

Noyes. Under the prolonged use of a mydriatic, rest and smoked glasses, to be followed by a carefully selected correcting glass, I have repeatedly seen these eyes get well. I believe not only that such treatment is fruitful of good results, but very important in the early history of macular inflammation of this class; and that such treatment will do much to avoid the serious macular disease so frequently seen in eyes with progressive myopia.

DR. EMIL GRUENING, of New York.—My experience agrees with that of Dr. Derby. Detachment of the retina in cases of this kind must be very rare. I think that we have here an inflammatory and adhesive process by which the retina and choroid are glued together. At one time I saw a patient who had such changes at several points in the eye, and between two points the retina became detached. Dr. Knapp also saw this patient and treated him finally. Dr. Knapp made a favorable prognosis in that case, and the retina actually became re-attached. The points of adhesion prevented the further extension of the detachment.

DR. EDWARD JACKSON, of Philadelphia.—As bearing upon the subject of the paper, I may say that I have two cases in mind, cases that I see or hear from occasionally, which I hope may prove the truth of Dr. Derby's observation. In one of these cases there has been extensive loss of vitreous of one eye; one has moderate myopia, the other hyperopia, but in either eye there are these choroidal changes going on. I have regarded them as probably the precursors of detachment, but I hope that that is incorect. I cannot recall any case in which decided choroidal pigmentary changes have been followed by detachment.

In both of these cases that I refer to, the changes are perhaps a little different from the ordinary mass of changes of choroiditis in this, that the amount of pigment deposit is relatively large, the changes are decidedly greatest in the macula, and the amount of atrophy of the choroid is small; in one I think there is no atrophy at all.

DR. B. ALEXANDER RANDALL, of Philadelphia.—There are,

of course, a good many types of this central choroiditis beside the form spoken of by Dr. Risley, which we have often seen and treated, which has been described as ametropic, and which is certainly in some cases curable. You have, of course, all seen the severe—and such cases have been described which are evidently beyond any help—types of this disease, and we have also seen those cases which have been described as central senile choroiditis, which become more and more marked as time goes on and which are in like manner irremediable. I would like to call attention to a point which is usually overlooked, and which is probably as rare in the experience of others as in my own, where we have a flask-shaped incision of the retina such as Jaeger has pictured. Of course I have seen but one or two cases. Jaeger shows one that is central at the macula and flask-shaped at the retina, but he does not in his picture represent what I have observed, a lesion of the choroid or pigmentary layers. It is possible we may have the retinal detachments in such cases not tending to progress, but going back to a normal position.

Paper by Dr. P. A. Callan, of New York, "Case of Orbital Traumatism, Followed by Immediate Monocular Blindness Due to Fracture of Foramen Opticum."

DR. ST. JOHN, of Hartford, Conn.—In connection with this paper of Dr. Callan's I should like to report a case of orbital traumatism unconnected with fracture of the orbit followed immediately by monocular blindness on the same side, for the reason that similar cases may happen in the experience of any of us—cases coming under our charge from a general surgeon.

In this case an operation had been done by a most excellent surgeon for the relief of tri-facial neuralgia; excision of the supra-orbital and infra-orbital nerves had been practiced and this was performed by a modification of the usual procedure which had been described at the last International Congress in Berlin last year.

The patient was an elderly lady, æt. 75, and in fair health. The operating surgeon found that there was more hæmorrhage

than was usual. He had operated, at the same time, upon the infra-orbital and upon the supra-orbital nerves.

Although the hæmorrhage was extensive, as I have said, it was not sufficient to cause him any alarm or anxiety. He closed the wounds and there was no special reaction. On the following morning the patient said she could not see. He noted that the pupil was dilated and sent for me. I found the pupil, on examination, dilated *ad maximum*, absolutely irresponsive to light; she had no perception of light, whatever, and no mobility of the eyeball in any direction. The immobility was just as complete as in any case of ophthalmoplegia externa. There was no plus tension; no glaucomatous condition. The ophthalmoscope showed congestion of the choroid and retina.

In the course of a few days the mobility of the eyeball began to return, but perception of light did not return, and has not to this year. This operation was performed four months ago. Mobility came back gradually, and the pupil contracted to perhaps two-thirds of this former dimensions, although remaining very sluggish to light.

My explanation—the only one I have to offer—was that this free bleeding had led to a clot, collecting at the apex of the orbit and involving the nerves running to the different muscles, as well as those surrounding the optic nerve, and that the contraction of the clot had caused the paralytic symptoms which followed. The case went on to atrophy of the optic nerve, which is now complete. I advised the gentleman who did the operation to re-open the wound, which he did, and syringed the orbit with a long slender-pointed syringe, hoping to break up the clot which involved this mesh of nerves at that point, and, if possible, free it from the contraction.

Whether the explanation I offered was the true one or not, I do not know. Perhaps as these operations are being done more frequently in these days it would be well to bear in mind to mention to our surgical confreres that they must be very careful with regard to cases in which there is free bleeding, and to relieve the orbit from the blood which collects there.

DR. MYLES STANDISH, of Boston.—I have had two cases in

my experience which are recalled by the paper just read by Dr. Callan, which I would like to mention as it may serve to relieve or prevent some member giving a false opinion some day. Each of these were medico-legal cases, one a young woman, the other a young man, who received an accidental blow and who declared that they were blind in one eye, and could see nothing by it immediately following the blow. In each of these cases there was reaction of the pupil to light and there was absolutely nothing to see in the fundus. I saw them both immediately after the injury at the instance of parties who were supposed to be responsible in each case. I said I believed that the patients were malingering. To my confusion and regret in each case three or four months after the injury atrophy, complete and perfect, ensued and the patients were absolutely blind. Of course, later there came on a dilatation of the pupil. But the thing that I wish to call attention to is that there may be a truthful statement from a patient after he has received an injury that he cannot see, though he may still have reaction to light; and that that injury may subsequently produce complete atrophy in that eye.

I remember also the case of a captain of an English steamer who fell into the hold of his vessel striking on the back of his head, and who, immediately following, had no vision in the right eye. I saw him within three days after the accident, but in his case there was the paling condition of the nerve, and following the accident there had been a sub-conjunctival hæmorrhage. In this case there could be no mistake. As to the other cases, I do not undertake to explain their nature.

DR. DAVID WEBSTER.—The cases cited by Dr. Standish recall to my mind a somewhat similar case in which I was called in consultation about a year ago. A young married man had insured in an accident insurance company and the loss of his eyesight was equivalent to the loss of his life; that was, he would receive ten thousand dollars if it could be proved that the loss of sight extended over sixty days.

The accident happened in this way: He was going rapidly through the door of a barn, jumping through, and struck the

top of his head against the upper part of the door and was severely stunned. He had certain nervous symptoms; went home and vomited and within a day or two lost his sight entirely. He was seen by a friend of mine from time to time, who thought he was malingering and after several weeks had passed I was called to see the case and asked to give an opinion. I found the pupils perfectly responsive to light and of normal size. I found no lesion of the fundus whatever. I found nothing whatever to account for this blindness. Yet he certainly gave every outside evidence of blindness, certainly acted as a blind man would act, and there was every reason to believe he was blind. I accordingly gave that opinion. I happen to know that the man got his ten thousand dollars, but I have not heard whether he recovered his sight after receiving it or not.

DR. SAMUEL THEOBALD.—In connection with the case reported by Dr. St. John, of ophthalmoplegia following section of the supra-orbital nerve, I think it worth while to mention that I have seen one very marked case of right hyperphoria following and persisting after resection of the infra-orbital nerve on the right side. It was done by a very careful surgeon in Baltimore and there followed diplopia and very marked hyperphoria. When I last saw the case the hyperphoria was diminishing. By correcting the marked refractive error which was present and by decentering the lenses, vertically the diplopia was done away with. If my memory serves me there were, when I first saw the case, seven degrees of hyperphoria.

DR. R. J. MCKAY, of Wilmington, Del.—I would like to ask Dr. Callan if there were any symptoms of orbital cellulitis in his case.

DR. CALLAN.—As far as I could see there was no indication of it.

DR. GEORGE C. HARLAN, of Philadelphia.—With regard to these cases which have just been cited of blindness coming on as the result of a blow and malingering blindness, you may remember that two years ago I reported a case to this society of monocular hysterical blindness which had lasted ten years

following immediately upon a blow which the patient thought had completely destroyed vision.

There was no reason to doubt that the patient was entirely honest in his belief that the eye had been blind for ten years and he was greatly delighted when the mistake was demonstrated to him. There has been no return of the blindness since.

DR. SAMUEL D. RISLEY.—I have observed four cases of monocular atrophy of the optic nerve following blows, three received on the orbital ridge and one on the top of the head. In the last case the patient, a man in middle life, was walking through a building in course of construction when a scantling fell upon his head. He did not consider that he was seriously injured, but in a few weeks sought advise for failing vision in one eye, which proved to be due to commencing atrophy of the optic nerve which subsequently became complete.

Of the other cases, one was a boy, æt. 12, who came to the University Hospital, having fallen, while running, upon the end of a stick he was carring in his hand. The blow was received on the infra-orbital right. In a few weeks there was failing sight and obvious paling of the optic nerve, which went on to complete atrophy and blindness.

Another case was that of an Irish carter, quite an old man, who was thrown from the cart striking the outer end of the left supra-orbital ridge against the curbstone. The injury was severe, and was followed by great swelling and œdema of the soft parts. When this had subsided he came to the University Hospital because of his failing vision in the left eye. He had commencing atrophy of the optic nerve which also became complete, ending in total loss of sight.

The fourth case I saw in consultation with Dr. Elmer of Davenport, Iowa, in July 1. A few weeks before a farmer had been thrown from a machine for loading hay in the field, and one of the prongs or fingers sharpened into a long cone had struck the outer angle of the left orbital ridge, glancing backwards. How deeply it had penetrated could not be determined, but he had complete secondary strophy of the optic nerve and

there were the remains of extensive hæmorrhages in the retina. In this case the iron finger probably either penetrated to the orbit or splintered the orbital walls, leading to extensive post-ocular hæmorrhage or possibly direct injury to the optic nerve.

Paper by Dr. R. A. Reeve, of Toronto: "A case of Pulsating Exophthalmus."

DR. GEORGE C. HARLAN.—I think that as ligation of the carotid is a serious operation, and not always a successful one, intermittent compression, being free from serious inconvenience, should be always given a fair trial. My own experience extends to one case only, but was very satisfactory.

The patient had very much the same symptoms which have been described in the paper. The injury was inflicted by the head being caught between the bumpers of two railway cars, and very nearly ended in death. Some months afterward the man came under my charge. After trying continuous compression by means of relays of students for twenty-four hours without material improvement. I had intermittent compression kept up from time to time upon the carotid, and afterwards the patient after returning to work was taught to keep up intermittent compression himself. He arranged an apparatus, a stick about a foot long with a padded end, by means of which he was enabled to attend to the compression very satisfactorily. He went back to his work on the train and whenever he had an opportunity he went into the baggage car, sat on a trunk and kept up the compression upon the carotid by holding the end of the stick against the artery. Some six months afterward he wrote to me that he was entirely cured and sent me later on his photograph to prove that the exophthalmus had entirely subsided.

This case was reported to the society fifteen or more years ago.

Paper by Dr. Emil Gruening, of New York: Divergent Squint and Its Operative Treatment.

DR. DAVID WEBSTER.—Does the treatment shorten the muscles on both sides?

DR. GRUENING.—No sir.

DR R. J. MCKAY.—I wish to thank Dr. Gruening very much for this paper. I operated two years ago on a case of monocular amblyopia, the result of an operation in early childhood in England, in which there had ensued an ulceration and destruction of more or less of the sclerotic coat over the site of the operation for the convergent squint, and you could see through the conjunctiva the bluish choroid very plainly. I did a complete section of the externus of the eye. I measured it the best I could. It was about 58 degrees with prisms. The patient occasionally had diplopia that annoyed her somewhat. I stitched the eye after the section to the nose, and kept it so for from twenty-four to thirty-six hours. I apparently gained one-third, but subsequently it diverged again and I did not gain quite so much. She went to Moorfields last year, for further operation, but she was not operated upon.

DR. GRUENING.—The operation I mentioned is not applicable to this case. It is one of secondary divergence. Here the motility of the eye inwards is reduced. In cases of ordinary divergent squint, the inner edge of the cornea can be moved to the caruncle and the outer edge to the outer canthus.

DR. DAVID WEBSTER.—I merely wanted to say that I have done Dr. Gruening's operation for divergent squint three or four times, I think four times, and in all cases with satisfactory results. I divided the externus and tied the eye, both externi, and tied the eyes together across the nose, and left them in a slightly convergens position. I think it is always necessary to bear in mind the convergent position. Moreover I think if you leave them too much so you get a hyper-correction. I have not yet in any case had to exsect any portion of the muscles. I wanted to give this experience because I suppose that very few members of the Society besides Dr. Gruening have had much experience with this operation.

Paper by Dr. Swan M. Burnett, of Washington: "Contribution to the Study of Heterophoria."

DR. R. J. MCKAY, of Wilmington, Del.—Dr. Burnett's paper was a very interesting one to me. I have done a good many

tenotomies and the more I do the better I like the results. I have done tenotomy on muscles the second time, and I concur with most of the views promulgated by Dr. Stevens. I am using many prisms in combination with sphero-cylinders.

DR. HENRY D. NOYES.—I think I might have written that paper of Dr. Burnett's and put my own name to it.

DR. BURNETT.—Thank you, Dr. Noyes.

DR. NOYES.—Most of the things which he describes are entirely in accord with my own experience and belief. The tests that have been spoken of are tests for distance and not for the near. Furthermore, the prisms have a value even if with limited range. Last year I was able to clearly show that they had no value in some cases and that tenotomies were preferable. But very often prisms have a value, which I think will sooner or later be found by other observers than myself.

I have no objection to the modern nomenclature in general, but I do object to one implication which they contain when we use the terms heterophoria or esophoria or exophoria; in that those terms are applied to the artificial condition as caused by producing vertical diplopia. If we are to rely upon the normality or abnormality of the muscles according to whether vertical diplopia being produced the images stand to the right or left I can assure the gentlemen present that they will fall into trouble. It is not a reliable test. It is only a test valuable up to a certain point, and, if you are to get a true idea of the right conditions, you must make the abduction and adduction test and also that for vertical diplopia, otherwise you will find great errors.

Dr. Burnett has operated on cases upon which I think no one would for a moment hesitate to operate. If I saw a case of esophoria or exophoria amounting to what he describes, I certainly believe that would come within the domain of surgery, and you can always calculate on good results. One thing more. The more I deal with these cases—and perhaps I have had more experience with them than most practitioners present—the more sure do I become that the essential element is a spasm of the muscles rather then a weakness of the muscles.

And I can account for many features in these cases upon the assumption of spasm which I cannot account for on the ground of paresis. The conditions may combine and for that reason I am certain that the temporary benefit which results from a partial tenotomy is accounted for on this hypothesis. But I still hold that a total tenotomy performed with the utmost care is better than a partial tenotomy. The reason lies in these anatomical facts, and these anatomical facts have not yet found sufficient credence among ophthalmologists, that the strength of the extrinsic muscles of the eyes depend, not only upon the strength of the muscles, but also upon the ocular fascia. I would advise the members present to consult the work by Motais, a volume recently issued, a big thick book. There are chapters in it which show you how variable is this structure, and how greatly it influences the action of the muscles. For that reason a tenotomy will be found in certain cases to have no effect, in other cases it will have a good deal of effect, but you must always reckon with the extent and density of fibrous tissue; it depends also upon whether the sub-conjunctival tissue is disturbed. It is a question of technique.

These are some of the suggestions that I wish to offer, and I am glad Dr. Burnett is coming out upon the plane where I find myself.

DR. SAMUEL THEOBOLD.—I wish to say a word with regard to Dr. Burnett's having performed only a partial tenotomy in a case in which he mentions the presence of eight degress of esophoria. My own experience would have warranted me in similar circumstances in doing a complete tenotomy of the internal rectus, with the expectation that I would eventually have to do a free tenotomy of the internus of the other eye. I have in mind a case. I do not remember the exact degree of esophoria, but I think it was not more than eight or possibly ten degrees at twenty feet, in which I did just such a tenotomy of the internal rectus of both eyes, first upon one and later on the other, and there still is a marked degree of esophoria left. The performance of partial tenotomy for esophoria amounting

to anything like eight degrees would in my experience be an ineffectual measure.

I would ask Dr. Burnett if I understand him correctly when he said that he gave prisms for one eye base in, and for the other base out.

DR. BURNETT.—No, sir.

DR. B. ALEXANDER RANDALL, of Philadelphia.—As to the use of prisms I would like to say one word for the bridge that has thus far generally carried me over, and I have to thank Dr. Risley for the pair of prisms he gave me several years ago, thereby very greatly increasing my ability to use my eyes and relieving my muscular asthenopia.

I would like to ask those who do much in correcting small degrees of heterophoria whether there is any real value in testing at the moment of operation the conditions of the heterophoria, which remains after the snipping of the whole or a part of the tendon has been accomplished, because I have seen results so nugatory as to make me doubt very much whether such tests carried on at the moment of traumatism had any value. It seemed that if a man did his tenotomy in accordance with what he had previously found, he did as much division as he judged right with approximate accuracy but that, if he undertook to correct that operation or greatly reduce it in accordance with what he then found, he was walking on a quicksand.

DR. GRADLE, of Chicago.—The occurrence of heterophoria has been so much emphasized by some, and, on the other hand, denied by so many others, that I have thought it best to test for its existence by other means than the prism test. I have devised a method for the purpose which I demonstrated at the International Medical Congress last year, and which is comparable to the haploscopic method of physiologists. A partition held projecting from the nose divides the field of vision of one eye from that of the other so that the position of lines in the separate field of vision of each eye may be compared at either a large distance or at a short range. The patient reads off directly the degrees which the eye deviates,

if any, from its proper position. In this method no optic device like prisms is used, thus avoiding accidental sources of error. On examining fifty normal and pretty nearly two hundred asthenopic sets of eyes, I found that minor degrees of latent squint are exceedingly common, but without any morbid significance whatever, if they do not exceed a certain latitude. The numerical indications furnished by this method are very constant at different examinations, and generally a little less than the deviations found by the prism test. Moreover, the deviations by my method are expressed in angular degrees. Deviations in a vertical direction I found to be the same for distance or short range. Horizontal insufficiency, however, varies with the distance at which the object is held. I have learned that vertical heterophoria of less than one or possibly one and a half degrees is a very frequent deviation which does not ordinarily lead to annoyance. Similarly an exophoria of less than two or three degrees for the distance I should consider harmless, while for short range it may account to six or eight degrees without causing any inconvenience. Latent deviations of sufficient intensity to cause either asthenopic or nervous symptoms have come very rarely indeed under my observation. I cannot but regard them as an uncommon occurrence, although they do not exist. But, if of sufficient intensity to cause morbid consequences, the squint is apt to appear manifest at times as well, and is extremely easy to recognize by means of my method as well as by any other mode of examination. Prisms I have not found satisfactory for the correction of heterophoria. A fact previously noticed by Dr. Stevens is that the correction by means of a prism although complete to-day, may appear insufficient to-morrow; in otherwords, that under a correcting prism the deviation may continue to increase in extent.

Dr. Noyes' view that spasm is the cause of these deviations would seem to be supported by this observation. My surgical experience in the correction has been limited to very few tenotomies, and those only incomplete and nerve partial.

DR. MYLES STANDISH, of Boston.—This subject is one to

which I have paid a great deal of attention for the last two years since reading a paper before this Society with regard to this particular matter. I must say that my conclusions are absolutely those of Dr. Burnett; that there is a very considerable proportion of cases of ametropia who have no insufficiencies of the muscle; that the greater quantity of those cases are cured by correcting the error of refraction, and that in the few that are left it is advisable to do a partial tenotomy. Two years ago I reported to this Society my five cases. I have done quite a number since. Of these five cases all but one had been through the hands of many members of this Society; they had been for seven or eight years incapacitated for their work; had been in the hands of neurologists; had not been improved; had been operated upon by me for partial tenotomies, and were treated by suggestion as several members of this Society intimated at that time. Seeing the title of this paper on the bulletin before I came to Washington, I took the trouble to write those cases to find how they had been doing since, and I am happy to tell the members of the Society that the suggestion made to them has lasted ever since; that these are well, all of them about their avocations, all marvels to their friends and all that sort of thing.

I think that there are a number of things to be considered with regard to these partial tenotomies. In the first place we do not want to do a partial tenotomy because we find insufficiency; in the next place we must remember that our insufficiency may be a spasm as Dr. Noyes suggests, or it may not be as Dr. Noyes did not suggest. Now, if you take many cases of insufficiency and examine them the first time they come into your office, you find that there is a large amount of insufficiency. If you undertake to correct their error of refraction by atropine and then examine them, you find a different sort of thing altogether. You now find very much less insufficiency and insufficiency in the opposite direction. Therefore, haste in this sort of thing is very ditastrous.

Now it seems to me that Dr. Noyes is perfectly correct in many of his statements upon this subject.

Dr. Maddox, of Edinburgh, suggested the glass rod for insufficiency in connection with the same thing as that suggested by Dr. Burnett. The glass rod makes no diplopia, horizontal or vertical, but shows insufficiency without any prism imposed. It is superior to anything we have had up to the present time. I think that the measurement of the amount overcome by prisms, the amount or the power to be overcome by prisms is utterly fallacious. I said two years ago I was not inclined to place much dependence upon it. Now I say I place no dependence upon it whatever.

DR. P. A. CALLAN, of New York.—I want to make a few remarks in this connection. I speak with special reference to the externi. In my hands partial tenotomies two weeks after the operation have no results whatever. In simple tenotomy of the muscle where I merely separate the tendon at its point of attachment to the eyeball without any further dissection of the fascia, I will average about two degrees for twenty feet as a result.

DR. J. E. COLBURN, of Chicago.—I have the honor of being an accredited visitor to the Society, and with your permission I would like to make a few suggestions with regard to this matter. In my opinion, the unsatisfactory results registered and reported during the first years that Dr. Stevens' work was on trial relative to the value of the graduated tenotomy as legitimate operative procedures, were due to the failure of operators to recognize the existence of these small bands of attachment which have been mentioned by Dr. Noyes as binding the tendons to the ocular walls, and also extending anteriorly to the capsule of Tenon and the conjunctiva. These, unless cut, will also prevent the full result of an operation for a complete tenotomy. To prevent a failure in such operations, I use a divulser thoroughly separating such bands. In the higher degrees of heterophoria I have modified the operation for graduated tenotomy, *i. e.*, proceeding as in the graduated tenotomy. After cutting through the tendon, pass the needles of a double-arm suture through the cut-end of the tendon back and out to the conjunctiva at the distance the operator deems it advisable to displace the tendon, then terminate the section of the tendon as in complete tenot-

omy. The suture is then tied. The wound through the conjunctiva should be parallel to the border of the tendon. I so make this operation for manifest squint. The only modification is that of passing the needles further back through the conjunctiva. In following this method of procedure I have found it advisable to make advancement of the tendon in fewer cases, and there is also less danger of displacing the tendon thereby producing hyperphoria as the result of such malposition.

DR. J. A. LIPPINCOTT, of Pittsburg.—In testing for muscular insufficiency, I think it unwise to rely exclusively upon any single method. I have seen cases in which repeated examinations with the rod test revealed no abnormality, but in which either exophoria or esophoria was actually present, as demonstrated by the vertical diplopia test, and also by the subsequent history. I am satisfied that the abduction tests have decided value, especially if we employ them to estimate not only the resistance to double images—the staying power—but also the power of fusion of such images after diplopia has been induced. This measurement can be made in the following way:

In trying the power of the internal recti, for example, the power of the rotary prism is progressively increased in the usual manner until double images are evoked. Then, without removing the apparatus from the eye, the power of the prism is gradually lessened until fusion of the images takes place. A patient with decided exophoria may show a resistance to double images equal to, say 24°, but it will be found, as a rule, in such a case that fusion does not take place until the prism is reduced to a strength of 8° or 10° or even much less. This method of using the rotary prism has been found in my experience to considerably enhance its value.

DR. R. A. REEVE, of Toronto.—Dr. Lippincott has forestalled me in his statement of his procedure. I have done this same thing in many cases, and I believe that it is a valuable adjunct in the using of prisms. I am glad to see the evident tendency to conservatism in the discussion of the paper read by Dr. Knapp, and the conservatism which was indicated by Dr. Knapp's paper. I am sure that we should take into consideration before passing to operative work the etiological factor in these cases of want of co-ordination. I am sure

many of them are temporary, depending upon rheumatic conditions of some senile muscle, and under proper treatment they disappear—to recur again perhaps. Then again many of them grow out of want of similar conditions in the eyeballs; that is to say, a case of ametropia with astigmatism of varying degrees in the two not corrected is very apt to set up a want of similar movement in both eyeballs, which to-day may be esophoric and to-morrow hyperphoric. I think it is necessary to break up this possibly temporary condition by treatment or paralysis of the accommodation and all use of the eyes at a near point. Again and again my record books shows disappearance of slight degrees of esophoria and very much more frequently the disappearance of hyperphoria under the prolonged use of mydriatics. I am glad to see a tendency to conservatism, as I said before, in treatment of these cases of hyperphoria, so-called.

DR. EMIL GRUENING, of New York.—I rise only to say that the operation suggested by me is not one for periodic divergence. It is for absolute and constant divergence. For a divergence produced by an excessive tenotomy of the internus, advancement of the internus is necessary.

DR. SAMUEL J. JONES, of Chicago.—It seems to me that the conservative feeling pointed out by Dr. Reeve may be kept up in other directions. As specialists we are much too prone to proceed to our local treatment regardless of the constitutional condition, whilst not entirely disregardful of it perhaps, paying smaller attention to it than we should. I think we shall be able to be still more conservative by following a proper constitutional treatment and supplementing that by electricity, improving the nutrition of the parts, giving more tone, less spasm, and in that way obviating many operations that are now performed and thus diminishing the use or the necessity of prisms. I speak somewhat from personal experience in these matters. I believe the theory to be right. In my own practice, it has been satisfactory.

I want to emphasize the fact that we are apt to get into the habit of disregarding the conditions which lead to these consequences instead of going back to the starting point, removing those conditions to a great extent, and thus removing a part if not all of the consequences that come from them.

[TO BE CONTINUED].

CORRESPONDENCE.

MACON, GA, NOV. 4, 1891.

EDITOR AMERICAN JOURNAL OF OPHTHALMOLOGY: Referring again to the subject of homatropine, concerning which I had somewhat to say in the September number of your Journal, if you will allow me space, I will give you the results of the trial of homatropine in four more cases of refraction work. Of course the record of four cases no more settles the question as to the relative merits of atropine and homatropine than does one, or even four, swallows make a summer.

Recently, Dr. Casey A. Wood, of Chicago, noticing my remarks in the last journal, sent me some of the gelatine disks to which he called the attention of the profession in the June number of the AMERICAN JOURNAL. These disks each contain $^1/_{50}$ gr. homatropine and $^1/_{50}$ gr. cocaine.

(Messrs. Wyeth, of Philadelphia, had previously sent me some to try, but having lost faith in homatropine, I did not use them.) However, at Dr. Wood's suggestion, I resolved to use the ones he sent me, and give them a careful and fair test. There was enough for about four cases. In two of the cases patients were young lady school teachers, æt. 24 and 27. One of them proved to be hyperopic, and the other had a compound hyperopic astigmatism. The accommodation was fully paralyzed, and the ametropia satisfactorily corrected by inserting one disk in each eye, and repeating it in 15 or 20 minutes, as recommended by Dr. Wood. Neither of the patients could read 16 of Jaeger at 15 inches, and they both had their ametropia successfully corrected within 90 minutes from the time the first disks were inserted.

The next case I tried them with was a lady, æt. 37. She had some pretty positive symptoms of eye strain, and I used only one disk in each eye. I tested her for close reading in 60 minutes, and found that she could read Jaeger 16 and 14, at 15 inches, fairly well, and in 90 minutes she could easily read Jaeger 14 at 15 inches. I afterward found out that this lady was emmetropic.

Case No. 4 was a boy, æt. 13. He had considerable symptoms of eye strain, yet by no means so bad as to have what I still denominate "ciliary spasm." After using a disk in each eye and repeating it in 15 minutes, in 60 minutes I found that he could read some of Jaeger No. 14 at 15 inches, and could readily read No. 16; at 90 and 120 minutes from the first he could still do this. Under the mydriatic his vision was:

R.=$^{20}/_{XL}$ +, L.=$^{20}/_{L}$+. R. with +50 ⊂ +25 or 50 cyl. ax. 90.=$^{20}/_{XX}$. L. with +1=$^{20}/_{XX}$—.

I then dropped in a 4 gr. sol. of atropine, and had him to repeat it at bedtime, and again early next morning, when I again tested him and found that his vision was:

R.=$^{20}/_{LXX}$, L.=$^{20}/_{LXX}$; R. with +1 D.=$^{20}/_{XV}$; L. with +1.50 =$^{20}/_{XV}$.

Under the atropine he could not possibly read any of Jaeger at 15 inches; nor was there anything like the difficulty experienced in finding the proper glass, which I so frequently encounter when I use homatropine. I have, in a very few cases, found that patients (not myopes) could read 14 and sometimes 12 of Jaeger, while as fully under atropine (a 4 gr. sol.) as I could get them. I have quite frequently had them do so while under homatropine. Will some one please explain this phenomenon?

Is it because some persons have a better perception of form for small objects than others do? I certainly think that Dr. Wood's idea that the homatropine is better absorbed and less wasted in the gelatine disk form than by an aqueous or oily solution is correct, and I thoroughly believe that he has effected a very great improvement in the use of homatropine. The great question that still remains (to my mind) unsettled is: Will homatropine, or homatropine and cocaine as certainly paralyze the accommodation as a 4 gr. solution of atropine will? It has failed me so frequently during a 3 years' trial of it that I have little faith in it as a *reliable* mydriatic in refraction work. For the present I shall not write more on the subject, but I hope by writing this I will draw out the opinions of those of wider experience than my own.

Finally, will some one inform me if we have any reliable test of complete paralysis of the accommodation?

Yours very truly, R. O. COTTER.